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Symmetry, Forced Asymmetry, Direct Apprehension and Elective Modernism

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Crazily, Norris seems to think sociology is at war with philosophy; it is not. I respond to his hostile comments on the sociology of scientific knowledge, which was inspired by Wittgenstein, by explaining the need for symmetry in the explanation of scientific knowledge, methodological relativism, elective modernism and a number of other issues.

KEYWORDS: Sociology of Scientific Knowledge; Symmetry and asymmetry; methodological relativism; elective modernism

Symmetry¹

A good proportion of the population of Northern Ireland are convinced that in the mass the bread and the wine only *represent* the body and blood of Christ. A good proportion of those who live in the South believe that there is transubstantiation: that is the bread and the wine *become* the body and the blood. The sociology of knowledge takes it that the difference in view is not to do with the actual bread, wine, body and blood but the histories of the two

¹ I start from the problem of symmetry rather than from Norris's (2014) comments because mostly I have no idea what he is talking about. He seems to have constructed a world in which hatred or fear of philosophy motivates the arguments of 'sociologists' such as Bloor and myself. But David Bloor is a philosopher, and I spend a lot of my time trying to be a philosopher such that the first two chapters of my 1985 'major statement', *Changing Order* (Collins 1985), are philosophy (they may be bad philosophy), I continually claim that the source of most of my inspiration is philosophy, I publish quite a bit in philosophy journals, I have co-authored a number of articles and a book with philosophers and, more recently, *Phenomenology and the Cognitive Sciences* and *Philosophia Scientiae* have each published special issues on my work while the *Journal of the Polanyi Society* has published a special section. Also I am proud to be an invited contributor to Hubert Dreyfus's *Festschrift* though Dreyfus and I disagree about a number of things. As I explain below, I do have enormous scorn for what I call 'presumptuous philosophical bottom feeders' and indifference to the regular bottom feeders, some of whose antipathy to the sociological analysis of science is so much more marked than that of the physical scientists with whom I spend about half my academic life. It may be bottom feeders who Norris has in mind when he uses the term 'philosopher' in describing a world of vicious professional rivalry that is otherwise unrecognizable to me. Or it may simply be that if you disagree with Norris on a matter of philosophy he interprets this as you being against philosophy as a whole, or maybe Norris thinks the proper extension of 'philosophy' is restricted to justification of certain beliefs. The problem is confounded by the fact that people like Bloor, myself, and my co-author Martin Kusch, see no tension between doing sociology and doing philosophy whereas Norris sees them as mutually exclusive.

groups and the upbringing of their members and tries to show how these ways of life work; that is a symmetrical view. To use old-fashioned language that is still useful for this purpose, each side's views are given an 'external' (Ext) explanation. An asymmetrical view, in contrast, would preserve the Ext analysis for, say, the North but provide an 'Internal' (Int) account for the South. In the Int account the view of the Southern Irish is given a two part explanation: (a) *Reality*: During the mass the bread and wine really do change into the body and blood. (b) *Direct apprehensibility*: Transubstantiation is, somehow, directly, or near directly, apprehensible to Southerners but not to Northerners.²

Direct, or near direct apprehensibility is essential because if the truth of the matter is only evident via others' interpretations then it is the interpretation that is the crucial thing on both sides of the explanation and we are back with symmetry – it is upbringing that provides the interpretation and it would be interpretation on both sides.³

symmetry		asymmetry		impossible	
Ext	Ext	Ext	Int	Int	Int
x	x	x ←	√		
x	√	Int	Ext		
√	x	√ →	x		

Table 1: Symmetry right and wrong

In Table 1, crosses mean wrong while ticks mean right and the possible types of explanation in the case of symmetry and asymmetry are shown. In the case of symmetry, both sides are given an Ext explanation and both sides can be wrong or the left-hand side wrong and the right-hand side right or vice-versa. In the case of asymmetry, just because a side is given an Ext explanation it does not mean it is wrong. Thus, even though the sociology of knowledge would typically explain the Southerners' belief in transubstantiation by reference to their socialization, and even though that view was in fact the result of socialization, the view could still be correct – the bread and the wine might change. The symmetrical view simply has nothing to say on the matter. In contrast, in the case of asymmetry there is always one Int explanation and one Ext and the Int side must always be right. Therefore, it follows that the Ext side must always be wrong since there can only be one right. That the Ext side is forced to be wrong is indicated by the arrows. The third logical possibility, 'Int-Int' is impossible in practice since there can only be one right and Int-Int would imply direct, or near direct, apprehension of two mutually exclusive possibilities.

² The Southern Irish would, therefore, be said to be 'rational' or some such. (They could be said to be responding to the 'TRASPness' of transubstantiation – Collins 1981a.)

³ The need for direct apprehensibility is nicely brought out by Norris's gleeful repetition of Richard Dawkins argument about the absence of relativism at 30,000 feet. This is taken to show that asymmetry is forced in the case of aeronautical engineering since an airplane passenger would directly apprehend that at 30,000 feet, it is better to be travelling in a Boeing than a cargo cultist's model airplane. That apprehension is well-observed but, unfortunately for the asymmetrist, it says nothing about the apprehensibility of theories of aeronautical engineering since the passengers know nothing about them except what they have been told. In a similar way, I can directly apprehend (or close to directly apprehend) that if I were to swallow-dive from the table on which I am typing this onto the tiled floor of my kitchen it would be bad for me but this does not lead directly to the truth of Newtonian physics nor the science of medicine.

The symmetrical view, to repeat, says nothing about what is actually going on in the world outside of the experience of the actors. For example, in this case it makes no claim about whether or not transubstantiation actually takes place in the mass.⁴ All that is needed to sustain the Ext-Ext analysis is that there is no part 'b' – no direct, or near direct [hereafter 'direct'] apprehensibility. Either of 'Ext' explanations could overlie the truth of the matter. Thus, though I do not believe in transubstantiation this is largely to do with my upbringing and experience as a scientifically-minded, atheistic, jew (I am here giving an Ext account of my view of bread and wine), but I have never taken part in a Catholic Mass and, if I did, I am not sure how I would recognize whether transubstantiation had taken place in the absence of a lot of interpretation. The Ext-Int analysis, on the other hand, requires both 'a' and 'b' for the Int side and implies that the Ext account *masks* the truth of the matter. In asymmetry the Ext view must be wrong because the Int view is right by direct apprehension and, to repeat, there can only be one right.

In the case of most of the topics to which the sociology of knowledge can be applied, the symmetrical approach seems safer as direct apprehensibility (even near direct apprehensibility), is a tricky thing. Furthermore, asymmetry carries serious methodological perils. First, the analyst has to know which side is right and it can be that this decision emerges, unnoticed, from the *analyst's* upbringing rather than from direct apprehension and that the part 'b' – direct apprehension imputed to the actors – is only assumed rather than demonstrated. Second, it seems dangerous to be able to backslide from the difficult task of explaining world views by choosing, every now and again, to allow them to be their own explanation. I prefer symmetry for this kind of reason and I call the position 'methodological relativism'.⁵ In a phrase that might have set a record for being quoted out of context, I say that to adhere to methodological relativism in a suitably rigorous way the analyst should act as though the natural world does not affect what comes to be believed about it.⁶

But, for the sake of argument, let there be topics to which the sociology of knowledge could be applied where an asymmetrical approach forces itself upon the analyst – these will be called cases of FA (Forced Asymmetry). There are a number of well-know philosophical problems indicating that it might be hard to find what we might call 'sweet FA' but we don't want to get stuck quite so early so, to move things along, let us suppose that it is possible to directly apprehend that grass is green. Note we are ignoring things like 'grue' and 'bleen' and the fact that a lot of grass is not green and that we don't really know what grass is or what green is. In spite of these problems, let us take it that where one group says that grass is green and another that grass is red, an asymmetric analysis is forced. So we have found a way of imagining FA, at least for the sake of argument. But we have also had to acknowledge that FA is not philosophically straightforward.

⁴ Thus there is no question of conflating ontology and epistemology. It is true that in my writings prior to 1981, most notably in my 1975 paper, 'The Seven Sexes' (Collins 1975), I claimed to be doing the equivalent of what Norris describes as replacing ontology with epistemology but after 1981 I became agnostic about the philosophical argument, concluding that I had no idea how these things worked in the long term, and replacing it with a methodological imperative about how to study science as it presents itself to us. The current paper is a philosophical reflection on how a sociologist must think and analyse.

⁵ Collins 1981a.

⁶ Collins 1981b, especially p. 3, which should be read in the context of p. 54 and surrounding.

The problem is that Norris appears to think that science as a whole exhibits FA. This is obviously not the case in at least two respects. First, science tends to take a long time to apprehend anything with any certainty. Thus, I have been studying attempts to detect gravitational waves with terrestrial detectors – a big expensive bit of science – for more than 40 years and as I have shown at length in many papers and three books there is no FA. The difference between those who think or have thought that gravitational waves have been detected on Earth and those who do not is not a matter of direct apprehension on the one hand and failure to apprehend on the other. All parties argue in the same way using the same techniques and methods to try to prove their point. Second, and more profoundly, there is almost no direct apprehension to anything in science, most obviously modern science. To quote my rather long 2004 book, *Gravity's Shadow*, on the sociological history of gravitational wave detection:

Reflect on what you know for sure about the things that are the business of science. The answer is almost nothing. We are all the same in this respect; we all know almost nothing. How can I be so confident about writing this when you, reader, might be anyone, perhaps even a gravitational wave scientist? It is because even if you are among the best and most brilliant scientists in the world, you know for sure almost nothing more than the most scientifically ignorant of us when by 'knowing for sure' we mean knowing to the standards of scientific proof: direct and repeated witnessing. Areas of expertise are like crevasses: deep and narrow. Even the best and most brilliant scientists have directly witnessed real proof in only that tiny part of the natural world in which they are specialists—and it is not so clear what 'directly witnessed' means even for them, since 'witnessing', and this is becoming more and more noticeable nowadays, is merely the conclusion of a very long chain of inferences.

As for the rest of the natural world, scientists know about things in the same way as we know about things: from hearsay. And even if you are one of the scientists I describe in these pages—one of the gravitational wave specialists—you know most of what you know about even gravitational waves from hearsay; that sounds odd, but think about it! Nearly all the science you know you learned from the printed page, the lecture theatre, or other scientists' talk and actions. Even the results you know by so-called direct witnessing are tiny corks bobbing on a huge sea of trust—trust in the results of earlier experiments, trust in the colleagues who work with you, trust in the meters and the materials which make up your apparatus, and trust in the computers that analyze the experiment.⁷

In other words, for all of us, it's nearly entirely a matter of history and upbringing. In most science this is obvious because the facts are still being reconciled never mind being directly apprehended; but it is pretty obvious even where there is good consensus among the natural scientists.

There is a whiff of paradox here because I am saying that certain things, such as the fact that we know most of the science we know from hearsay, 'are obvious' – i.e. directly apprehensible – whereas I have just been arguing how rare and philosophically recalcitrant immediately apprehensible things are. I was all set to apologize for the compartmentalization involved until it occurred to me that something more profound might be worth pursuing.

⁷ Collins 2004, 4-5.

When we look for matters directly apprehensible we may have been searching in the wrong place: perhaps we shouldn't be looking for whether grass is green or even for a 'sensory atom' such as 'green here now'. Perhaps a better candidate for direct apprehension is 'this is how it is to exist in my society'. Such a claim, after all, is a report of one's day-to-day experience with no added synthesis. Nothing is that easy, unfortunately, and even this apprehension does demand some conscious reflection in order to avoid the overlay of mythical descriptions of experience that we live with, such as, for example, that the facts of science are known directly: one has to reflect to notice it is not so. Hence the need for the above passage from *Gravity's Shadow* which may, initially, have struck its readers as counter-commonsensical and which I know some philosophers find it hard to get their heads round – though most natural scientists of my acquaintance can.⁸ But a bit of reflection seems a small price to pay for something so apprehensible. What I am saying is that the paradigm for direct apprehension should not be sensory experience but social experience. Perhaps this accounts for 'the unreasonable effectiveness of participatory methods in the social sciences'.⁹

Bottom feeding

Proceeding to take methodology rather than philosophical principle as the crucial matter, let us consider the possibility that it might not be unreasonable to do asymmetrical studies of science if you wait long enough. A philosophical 'bottom-feeder' could wait until the facts of science had drifted all the way down through the multiple digestive systems of the big scientific fishes in the ocean of verification and settled in the sediment of uniform or near-uniform consensus. That consensus might, perhaps, be used as a proxy for a direct apprehension to which neither analyst nor actors have access. This is because anyone who does not accept that degree of consensus is close to being crazy albeit in a socially deviant way rather than a distorted vision way. For example, 30 or 40 years from now it could be that gravitational wave astronomy is no more remarkable than radio-astronomy and anyone who no longer believed in the detectability of gravitational waves could be reasonably treated in the same way as one would nowadays treat someone who did not believe radio-telescopes were really seeing stars (note that we, who firmly believe they are detecting stars, have mostly never looked at the heavens through a radio-telescope and, even if they did, would not know how to tell whether they were seeing stars or not). I don't think one would learn much from such an asymmetrical treatment but I would not know how to argue against someone who wanted to provide one except to say that it was not a very productive thing to do.¹⁰

⁸ It is not surprising that the scientists can handle the idea since they are continually involved in assessing whether or not to take others' empirical and theoretical claims seriously and it is, perhaps, clearer to them than to philosophers that direct apprehension is not going to help.

⁹ For years I have told the following story: My friend Gary Sanders, the then project director of the Laser Interferometer Gravitational-Wave Observatory (LIGO) was once ragging me about the weakness of my sociological methods which amounted, as he put it, to 'asking a few people what they thought and recording it as a finding if two or three of them agree'. At the time we were having lunch in the LIGO Livingston installation about 40 minutes drive from Baton Rouge, Louisiana. I replied that my methods were much more robust than his. Having never used public transport in Louisiana, I was ready to bet that I could not get on a bus in Baton Rouge and buy two tickets, one for me and one to reserve the seat next to me. I pointed out that our joint certainty about that was greater than his certainty would be when the first gravitational wave was discovered. It has taken all this time for it to occur to me that there might be something more going on here than a quip.

¹⁰ It could be – I am really not sure – that the idea of bottom-feeding relates to Norris's discussion of stratification in the relationship of ontology to epistemology; bottom feeders concentrate on one of Norris's strata.

The bigger trouble with philosophical bottom-feeders is that they have a tendency to drift upward through the sea and grab morsels that have not yet been fully processed and take it to be their business to eject them from the body of science. It is a contemporaneous and premature Whig history of science – premature ejection as we might say. For example there is a whole mob of ‘philosophers’ who grab the bits spat out when the big scientific fishes take the occasional ill-advised bite out of parapsychology and the like. Presumptuous philosophical bottom feeding is shameful stuff.

One thing I learned from the ‘Science Wars’

My first big engagement in what became known as the ‘Science Wars’ was a blazing row with Luis Wolpert at the British Association for the Advancement of Science; it was in 1994. One thing Wolpert said stuck in my mind, however. He said that in his field – embryology – one had to be reading new research papers every week to keep up with the field whereas in our field – science studies – nothing ever changed. And here we are again going over ground that was argued to death, as I thought, in the early 1980s; thus there was something in what Wolpert said.

And yet I do not think the charge sticks to sociology of scientific knowledge (SSK) and what followed anything like as firmly as it does to science-wars type activity itself. Of course, SSK was itself inspired and informed by philosophy, notably Wittgenstein and Kuhn, but I think I can describe the difference between it and the typical *philosophy of science* problem when the 1960s was turning into the 1970s and I was first becoming acquainted with it. Duhem, Quine, Hanson, Popper, Lakatos, and others were stymied by what we might call the ‘child’s picture model of science’ (CPM). When children draw their houses they sometimes put a strip of blue along the top – the sky – and a strip of brown or green along the bottom – the ground. In the CPM the equivalent of the top strip is theory and the equivalent of the bottom strip is findings; the job of philosophy of science was taken to be to show how they articulate. The clever philosophers argued that the two strips were not really independent (‘observations were theory-laden’ etc.) but progress was slow, consisting of incremental shifts from the CPM.¹¹ Sociology of scientific knowledge and the symmetrical approach sidestepped the CPM by simply studying the ways that people were persuaded to take this or that as knowledge, with science as a specially interesting and specially easily researchable case.

What then happened was extraordinarily rich. Through closely examining science with an attitude that caused one to ask, ‘How do they argue for this rather than that?’ rather than, ‘What would a rational person think?’ the whole world of science opened up. To brainstorm, we (the collective we – the science studies community), discovered the crucial role of tacit knowledge in carrying out scientific experiments and, consequently, the problem of the ‘experimenter’s regress’ and that replicability is as much consequence as cause of agreement over findings. We found the extent to which experimental findings were interpretively flexible and how this allowed ‘non-scientific’ interests to influence what counts as a scientific result. We found that fringe sciences and core-sciences were treated very differently by journalists in terms of the extent to which they accounted themselves expert (relationship between

¹¹ I have to admit I have not read as much very recent philosophy of science as I should, but it sounds from Norris’s description as though it is following the path that SSK has opened up, and that must be a good thing.

constitutive and contingent forums). We found that military funding for research followed the logic of Pascal's wager and was far more open than civil funding. We explored in detail the different logics and imperatives of small science and big organized science. We uncovered the 'literary technology', that gave rise to the scientific paper and more recently we have come to understand that this technology is equally effective whether applied by the Nobel laureate, the internet crank or the 'scientist' purchased to construct a case for the tobacco industry. We now see why knowing findings and theories is not enough to understand science; the scientific community has to be known as well. Confronted with the social basis of knowledge we discovered how some kinds of human action can be mimicked by computers whereas artificial intelligence still awaits the breakthrough that will allow computers to become social if the other kind of action is to be copied. We discovered the process by which 'ships are put into bottles' or scientific ideas are 'black boxed' so that disputes become very hard to re-open and we found that some of this has to do simply with the way findings are expressed in publications – the stripping of modalities. We discovered the 'core-set' of deeply involved experimenters and theoreticians and how it relates to the much larger penumbra of those who discuss and evaluate. We discovered that 'distance lends enchantment': the penumbra is far more certain of its knowledge than those in the core-set and that is why as knowledge moves from the core to the outside rings debating turns into campaigning and this, in turn, explains a huge amount both about how science works in its interaction with policy and the public and how our own fields carry out their business – not least science wars. We discovered the idea of 'evidential significance' – that the same findings could be interpreted in ways more or less portentous with very different risks and consequences. We discovered the difference between evidential individualism and collectivism where in the first kind of science each person is considered responsible for their own findings and errors whereas in the second kind of science potential findings are broadcast and assessed in public by the whole scientific community. We found that technologies were as differently interpretable as scientific results and that development trajectories were affected by meaning rather than the logic of materials. We uncovered the relationship between testing a device and using it and we found the difference between an experiment and a demonstration. We discovered the importance of 'interactional expertise' and how expertises could be classified and distinguished and we found how to subdivide tacit knowledge into kinds. We found out the many ways in which interdisciplinarity can and can't work and so on. This list covers only things that have the potential to circulate outside of specialist and inward-looking scholarly boundaries, spreading to other communities and appreciated by physical scientists, by knowledge engineers, by computer designers and testers, by music pioneers and by policy-makers. Somehow, these outcomes do not seem, as Norris claims, to be the symptoms of a disease.

Three waves of science studies and elective modernism

Returning to methodological perils, in the case of asymmetry, the analyst knows (or believes they know), which side is right and which side is wrong at the outset of the analysis. Furthermore, the analyst has to believe that the rightness of the right side is directly apprehensible (part b). This is quite different from symmetrical analysis where the analyst does not know who is right and/or is careful not to make any such assumption or align the supposed rightness of one side or the other with the analyst's own view. In the case of symmetry the analyst finds an Ext explanation for all views whether right or wrong; in the

case of asymmetry the analyst has to find an explanation for why those on the Ext side do not apprehend the obvious truth of the matter. Explaining turns to blaming! To explain properly can be hard work as the many case studies of scientific dispute reveal – one has to get right inside the world view of those who do not apprehend the world in the same way as the analyst.¹² It is, therefore, tempting for the analyst to take a short cut and guess at the explanation. Norris's review of Bloor is useful in that it illustrates the process rather beautifully. Norris is convinced that his view of how science should be analysed is the correct one and the directly apprehensible one (at worst, a bit of reading will cure the affliction of the benighted Bloor, Collins et al. and convert them from the Ext side to the Int side). Norris then explains the initial reasons the afflicted find themselves on the Ext side:

Any honest or clear-headed answers ... might well have to do with such prickly matters as cultural capital, professional self-interest, disciplinary rivalry, the job-market, physics-envy, philosophy-phobia, research-grant scarcity, and above all the chronic insecurity of social scientists *vis-à-vis* the natural and formal sciences.¹³

This is the kind of thing that can result from the felt force of the arrows in Figure 1. It is hard to imagine a lazier way of analysing anything. Below I find myself again in Norris's hands:

However he [Collins] has now become anxious about Phase Two, not least as a consequence of talking to medical experts on the topic of the AIDS epidemic in sub-Saharan Africa. ... Collins finds it inconceivable that the resultant problem might require some re-thinking of those Phase-Two premises and a readiness to allow the possibility that Phase-One thinkers like Merton got it right [and there is more such stuff].¹⁴

It is time to draw this to an end. There is no great change of mind going on here. Our book, *The Golem* became a *cause celebre* in the science wars because the Cornell physicist, David Mermin, took it to be attacking the truth of relativity.¹⁵ It was doing no such thing as the logic of symmetry makes obvious and as Mermin was happy to admit after he and his colleagues had talked it over with us. This can be seen in another book, the one that resulted from that discussion and which, I believe, ended the science wars (holdouts such as Alan Sokal and Chris Norris aside). That book is an edited debate between a science side (Bricmont, Sokal, Saulson, Mermin, Wienberg, Wilson, Barsky, Labinger) and a social science side (Pinch, Lynch, Gregory, Miller, Shapin, Dear, Collins) and is called *The One Culture*.¹⁶ And wave 3 of science studies was anticipated as early as the first edition of *The Golem*. Published in 1993, and written at least a couple of years earlier, on page 140 we find:

Let us admire [scientists] as craftspersons: the foremost experts in the ways of the natural world.¹⁷

¹² See, e.g., Collins and Cox 1976.

¹³ Norris 2014, 9-10.

¹⁴ Norris 2014, 27.

¹⁵ Collins and Pinch 1993.

¹⁶ Labinger and Collins 2001.

¹⁷ One can also find sentiments not too far from this around p. 54 of Collins 1981b.

I cannot speak for the whole science studies community but I know that me and my colleagues from the Edinburgh School and the Bath School have always been great lovers of science and that is why we wanted to understand it better. Nothing has changed in that regard and there is going to be still more friction with Norris's 'explanations' of why we believe what we do when he sees the next, Cardiff, instalment of the 'third wave' – 'elective modernism'.

Under elective modernism there is most certainly 'a readiness to allow the possibility that Phase-One thinkers like Merton got it right' and it is not 'downright unthinkable',¹⁸ as Norris, never shy about imputing an internal state to someone else, says it must be to people like us. What Collins actually believes Merton got right was the excellence of the value-system of science. What Merton was right about was that the values of science are good ones – in a morally absolute sense – without any need to justify them by reference to the efficaciousness of what emerges from them.

And that is the clear difference between people like Norris and people like myself – elective modernists. Norris only loves science and scientists that produce correct results; this is a very small proportion of science, identifiable in the long term at best. Elective modernists love all scientists including the ones that are wrong and working on material too early in its gestation for us to know how it will turn out. So long as they are seeking truth with integrity – so long as they are cleaving to Merton's and other scientific values – elective modernists love the econometric forecasters who get the economy wrong year-after-year; they love the rejected parapsychologists; they love the long-term weather forecasters; they love the mavericks who think they have shown experimentally that relativity is wrong; they love those who think that anti-retroviral drugs are poisonous and who led Thabo Mbeki not to distribute them in South Africa. And you have to be ready to love wrong scientists if you want to love science because most scientists are wrong. But to love these wrong scientists along with the right ones is not to give them parity of esteem for what they find. Eventually, just like any other citizen, the sociologist of scientific knowledge will prefer the science that he or she comes to use. Crucially, to love anyone with scientific integrity is not to treat all such people symmetrically when it comes to making policies that turn on science and technology. Today's policy-making is an immediate business, not a bottom-feeding business, and there is no choice but to make judgements about how the long term will turn out long before it arrives. Wave three argues that the *best* such judgments (which may not turn out to be the right judgments) are made by the wisest people – and that, of course, begs a load of questions and spawns a series of fascinating research projects. The elective modernist does not love Thabo Mbeki because Mbeki did not base his judgements on the views of what were, at the time, believed to be the wisest people, and the elective modernists would not advise that parapsychology or alternative medicine or anti-relativity views inform political judgements. The analysis of how policy should draw upon science is one thing and it should be asymmetrical; the analysis of how one thing rather than another comes to be believed to be scientific truth is another thing and to do it properly requires symmetry; as for scientists, irrespective of their results, some are bad people to be scorned but most have been professionally brought up to be good

¹⁸ Norris 2014, 35.

people and that is why science should remain central to our way of life. That is elective modernism.

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